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THE PHILOSOPHY OF MEDICAL SCIENCE.

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[Continued from page 74.]

BUT let us examine some points in Dr. Bartlett's theory more in detail.

I. As already noticed, it excludes from science all its fundamental principles, or primary truths. There can be no doubt that he excludes them. They are not phenomena, they are not even generalized phenomena; nay, more, they are not even the deductions of reason from ascertained phenomena. They are primary truths lying at the foundation of all reasoning and observation. They are truths discerned by the mind in the exercise of its higher powers of "original suggestion," as Reid, Stewart, Brown, and some of our most distinguished American philosophers, express it; or of the "pure reason," as Kant, Cousin, and the Continental philosophers generally, and Coleridge, and some American philosophers, express it. Cudworth and Locke expressed the same view, in other words, though it is not in accordance with the leading views of Locke's philosophy. However much philosophers may differ on other points, whatever obscurity and mist may hang over other parts of their philosophy, however wild many of their speculations may be; on this point, viz., that these "primary truths," together with certain "primitive ideas," are not observed by the senses, and are not deduced by the reasoning power, but are directly discerned by the mind in the use of an intellectual vision which it has for such truth, in the exercise of its higher power of intuitive perception, or original suggestion—that the pure reason has an eye that can see them directly, the moment they are brought within the range of its vision—on *this* point they are all agreed, they are all clear and definite in their statements, and their views receive the ready assent of sound, thinking minds. It is only such philosophers as Hobbes, Hume, Gassendi, Mill and Comte, and Condillac, who hold a contrary opinion. Now all such primary truths, which are not observed phenomena, which are not phenomena in any sense, are, of course, excluded from science by Dr. Bartlett's theory.

But, inasmuch as he has referred to some of these truths (pp. 26 and 79), to the principle that "every change has a cause"; that everything peculiar in the cause involves a corresponding peculiarity in the effect; that everything peculiar in the effect, implies a corresponding pecu-

liarity in the cause; and that the phenomena and processes of nature are uniform and invariable; since he has referred to these fundamental principles, and admitted them as "universal and necessary,"* and seems to regard them as antecedent to science and essential to it, but not included in it, we will not dispute this point with him. If he chooses to use the term "science" in a more limited sense, as embracing only the phenomena observed, and the facts and truths and ideas arrived at by the thinking, reasoning mind, by the aid of these phenomena, and these primary truths, so be it.

Still, these fundamental principles, being essential to science, being inseparably connected with all its observations and all its reasoning, it would seem most fitting, in a broad and comprehensive view of the philosophy of science, to include them among its proper and essential elements, as elements so essential, that without them science could not exist. Geometry does not discard its axioms, why then should the science of life discard its fundamental principles.

But we cannot consent to go farther, and limit science to mere phenomena, banishing all facts proved by reasoning, and all the ideas, truths and principles which constitute its higher elements.

This leads to the second point, that

II. Dr. Bartlett's theory excludes all those facts which are not observed directly, but are proved and conclusively proved by reasoning; and moreover leaves no room for the employment of the reasoning faculty.

This lies on the very face of his theory, being one of its principal features. He maintains expressly that no fact in science can be proved by reasoning, but must be observed by itself if known at all. His proposition is, that "these facts, phenomena and events, with their relations, can be ascertained only in one way, and that is, by observation, or experience. They cannot be deduced or inferred from any other facts, phenomena, events or relationships, by any process of reasoning, independent of observation or experience." He says, again (p. 17), "Each distinct and peculiar relationship can be ascertained in one only way, by one only method, that of observation of the relationship itself." In other places (pp. 10, 75, 76), he maintains, that "Each separate class or series of phenomena or relationship must be observed by itself," "that a knowledge of one class cannot be deduced or inferred from the knowledge of any other class, by any process of reasoning."

Now there can be no doubt that there are some facts which cannot be proved by certain other particular facts. He has cited a large number of such instances. And this is the only proof he has brought, of the correctness of this part of his theory. He has shown conclusively that

* His remark in this connection (p. 27), that "all exceptions to this invariableness and uniformity are apparent only, and not real," is a very just one. But his subsequent remark, "that the old saying, so constantly and blindly repeated, 'that the exception proves the rule,' is as destitute of truth as it is of meaning," is not just. The "exception," though only apparent, does prove the rule. Did not the rule exist, the exception would never be made, would never be thought of. Were there no true coin, there never would be counterfeits. Counterfeits prove the existence of true coin. Exceptions (though apparent only) prove the rule. Viewed in this, its true light, the old proverb is full of significance.

some facts cannot be proved from *some* other facts (as, for instance, that the structure of the heart does not show the nature of the blood (p. 81 at bottom) ; and that is all he has shown.

But in the preceding and in many similar statements of his theory on this point, he means more than this, though he has proved no more.

He starts with the idea, that there are certain classes or series of things so like each other, that in observing one or one hundred of them, you actually observe the whole series at once ; that each and all the individual facts in the series are observed in a lump in that one observation. Exactly as certain theologians are supposed to have believed, and perhaps some of them did believe, that each and all the individuals of the human race sinned together in that one sin of Adam. The cases are exactly parallel on the point in question, and one is just about as true as the other. However, assuming this idea (for it is a mere assumption), our author is able to bend the facts of nature to his theory, by which, in order to exclude speculation, he would exclude all reasoning from science, and allow the mind only to observe phenomena, to see their resemblances, and differences, and put those which are alike together in proper order, in all which no true process of reasoning is involved. He does not take the true view of the case, that by means of a certain number of classified phenomena, the human mind in the exercise of its reasoning faculty, and by a genuine process of reasoning—of true inductive reasoning, from particulars to the general truth—is enabled to ascend from the material facts to the truth, the general truth implied in those facts and proved by them, though it embraces all other similar facts which are yet unobserved ; and then, from this general truth thus proved by reasoning, the mind descends again by a genuine process of reasoning, though in a reverse direction, from the general truth to the particular fact, and can arrive at any one of the facts coming under that general truth, if necessary. Such a fact thus known, is a proved fact, not an observed one, and this is the character of the body of facts known to science ; it is comparatively very few of them that have been observed—or rather, science chiefly consists in the general truths, which have been proved, and are ready to be applied to any particular fact when needed.

Instead of taking this correct view of the case our author has chosen, in consistency with his theory of "observation," to scatter to the winds all these reasoning processes and their results, to banish from science those truths and principles in which its glory and life and power and practical value consist, and reduce the whole to an act of observation, an act of seeing a whole class of facts at once, while looking at one of them. But this idea is an absurd one. We may indeed know a particular fact on finding it to be a logical consequence of a general truth proved from some other fact ; but we do not observe the one in the other. Just as a man (to extend our theological illustration) may commit sin in consequence of a general course of things, resulting from a previous sin of another person, as of Adam ; but he does not sin in the sin of that person, he must sin himself if he sin at all. And a phenomenon must be observed itself, if it be observed at all.

The facts, however, that we are now considering, are not observed in

any way; they are proved by a regular course of reasoning, and in every case the mind must pass through this course of reasoning, even though it dart through it with the rapidity of the lightning's flash. Genius, even, is not freed from this law. Goethe and Newton were obliged to arrive at truth by this method. A single fact, it is true, was sufficient for them, where a hundred would be needed by a common mind. With the keen eye of genius they were able to discern the truth when millions of facts had not revealed it to inferior minds. And probably they arrived at it in a moment. The reasoning process was gone through by their minds in an instant, though for the first time, as rapidly and with as great facility as the mind of the most rapid pianist passes through its processes after having gone over them a thousand times. When Newton saw the apple fall, when Goethe saw the vertebrate form in the skull of the deer—the law of gravitation, and the law of the structure of vertebrated animals, may have flashed into their minds in an instant. But the law of uniformity was the conductor by which the electric truth entered. There is a powerful attraction between such minds and truth; but the dazzling splendor of its instantaneous and brilliant results, should not blind our eyes to the mode in which the results are obtained. Their minds must move along the road which God has created for every mind to move in. They must pass through the inductive process, they must go in the path through which the law of uniformity leads them. The same process which other minds have gone through, to confirm their results, that same process their minds went through when they first attained them. The truth was then proved to their minds by reasoning, though even *they* did not dare to place it in the temple of science, till they had confirmed their result by many and varied repetitions of the same reasoning process. And they having led the way, thousands of minds have followed in their footsteps, each for itself verifying their results.

But to return to the theory of our author. While he, in his peculiar way, admits that all the facts of any one class may be ascertained, from the actual observation of a few of them, he denies that the facts of any other class can be thus ascertained without being specially observed by themselves. "Each class of phenomena can be ascertained only by direct observation of the phenomena themselves." Physiology cannot be deduced from anatomy—nor can pathology be deduced from physiology—nor can we ascend thence to therapeutics. Each class of facts must be observed by itself. Nay, each particular species of facts in each of these several departments must be observed separately. When we get down to those lower classes, those particular species of facts which are so exactly alike, that in seeing one we see all, something may be done. But above this, our previous observation is of no avail in aiding us to arrive at any knowledge of facts which have not yet come under our eye.

But, while each particular species of fact has its peculiarities, are there not also resemblances between the different species which enable us to arrange them in various genera? and these genera again, in various orders? and so on up to wider, more general, more comprehensive

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divisions? And is not here a sufficient foundation for reasoning from one class to another? Are there not also relations of cause and effect which enable us to reason in another way from one class to another?

It is not denied that there must be special observations in each department of our science, and in each class of facts, some to confirm the results of our reasoning (which must always be done where it is possible), and others to ascertain facts that reasoning cannot reach. Each class of facts has its peculiarities which require separate observation, unless we can reach them by reasoning from cause to effect, and *vice versa*. But there are such relations between the different classes of facts, and between the several departments of our science, that our knowledge of one department is in a great degree dependent upon our knowledge of the others, and much of our knowledge could never be attained in any other way. It is useless to follow our author through his long array of facts and argument on this point. It does not reach the question. Because some things cannot be proved from a particular class of facts, it does not follow that nothing can be proved from it. It is also useless to cite many instances. They are innumerable, and only a few obvious ones need be alluded to. Any one who has read the late investigations into the minute structure of the kidneys—of the manner, for instance, in which the tubuli uriniferæ and the bloodvessels come together in those wonderful corpuscles of Malpighi, should be slow to admit that nothing has been thereby added to our knowledge of the functions of that organ, even though the precise mode in which urine is secreted there, and the reason why urine rather than bile or any other fluid is secreted, is not known, and very likely never will be. But setting *this* aside, does our knowledge of the function of the kidney in no measure depend upon our knowledge of its anatomical relations? and upon its similarity in structure to other secreting organs? Take this anatomical knowledge away from us, and what should we know of the functions of this organ? By the aid of these anatomical facts we know that it *must* be the organ that secretes urine, and not the bladder or the ureters, or the supra-renal capsules. By reasoning from these facts, we know that the urine must be secreted in the kidney; but we never caught that organ in the act. No eye ever saw it in the exercise of its function. The well-known fact in regard to the proper function of this organ is a proved fact, and not an observed fact. The same is true of other organs. Even where we can see an organ in the exercise of its peculiar function, it is probable that our knowledge of it depends chiefly upon reasoning from its anatomical structure and relations. We know more and understand more of the function of the heart from our knowledge of its anatomy, than we do from its thumping against the walls of the chest, from the beating of the pulse, or the spouting of the divided arteries. And even those of us who have looked upon its curious movement, as seen in the opened thorax of a living animal, know very little more of its function than those who never saw it. Though, as our author says, the structure of the heart throws no light upon the nature of the blood, it does throw a flood of light upon the character and

mode of its own function. But perhaps we get a little light respecting the functions of the blood, from another source, from its own structure or constitution as revealed by the microscope and by chemical reagents.

Not to enter further into particulars, what should we know of the functions of the vesiculæ seminales, of the olfactory lobes, of the different parts of the internal ear, of the corpora quadrigemina, indeed of almost every organ or part of an organ in the body, if our knowledge of their anatomical structure and relations were taken from us. Is it not true that a large portion of the whole circle of our physiological knowledge is more or less dependent upon anatomy and reasoning, instead of being, as our author maintains, absolutely and entirely independent of both. He has taken the wrong view of the matter, he has "got hold of his pitcher by the wrong handle." In regard to therapeutics, it would be easy to show that the treatment of disease by our wisest and best physicians depends in a very great measure upon reasoning from cause to effect. There is very little specific treatment in the whole round of practice.

With reference to his exclusion of reasoning from science, it may be asked, what becomes of the method of reasoning by exclusion, by which we ascertain the character of a particular tumor, for example, by determining that it is not this, or that, or the other kind, thus excluding, one after another, the various forms of this disease, till we get to the right one, and thence arrive at the conclusion that it must be that particular form of tumor, because it can be no other. This is legitimate reasoning, and is in constant use. But is the fact thus arrived at, an *observed* fact? If so, it must be observed by *not observing*!—"lucis a non lucendo."

It will be noticed that (pp. 79 and 87) the author has felt obliged to refer to the aid we derive in science from the "law of uniformity," and the "law of causation." But in doing this, he does not modify his theory, or recede from his position, "that the only reasoning there is in science consists simply in the act of arranging and classifying." This is strange enough. Will the author tell us what process of reasoning there is that does not consist in the application of such laws as these, or some modification of them, to the facts of science? He has thus unintentionally, and contrary to his theory, admitted into science all the known processes of reasoning.

Let the mind take these laws and with them walk forth among the facts of science, and it will have work enough to do, and room enough and opportunity enough for the full exercise of all its reasoning powers. The author has admitted here all that could be asked, if he will only carry out his admissions to their full extent, and modify or remodel his philosophy so as to give reason its full scope and proper place in science, and allow it to bring with it all those facts, truths, and principles, which cannot be observed, though they can be most conclusively proved. Of the power of reasoning, it has been well said that it "appears to have been given us in compassion to our weakness, that we may acquire knowledge which otherwise would not be within our reach. It brings to light the great principles and hidden truths of nature, it gives grand and comprehensive views which could not otherwise be obtained, and in-

vests men and external things and events, in their origin and in their consequences, with a new character."

[To be continued.]

M. RICORD'S LETTERS UPON SYPHILIS.

Addressed to the Editor of L'Union Medicale.—Translated from the French by D. D. SLADE, M.D., Boston, and communicated for the Boston Medical and Surgical Journal.

EIGHTEENTH LETTER.

MY DEAR FRIEND,—In the positive inoculations, things always occur as I have told you in my last letter. When the inoculation fails, the puncture becomes a little irritated, but this soon disappears.

However, without depriving inoculation of anything which is established, we must recognize that there is for syphilis, as for the variola, and for vaccine, *false pustules*. Their existence, if the examination is superficial, might lead to error. My learned colleague M. Puche, confesses now, with a good grace, that he has been thus deceived by *false pustules*, when he formerly practised inoculations with muco-pus furnished by balano-posthitis. Thus, he does not attach to-day the same value as formerly, to the facts contained in the *memoiré* which he has published upon this subject; he has studied these facts better, and they have for him changed their signification. You ought to understand that I should not have committed the impropriety of speaking thus, if I was not formally authorized by M. Puche himself. My critics, then, who have made much ado about the inoculations of the muco-pus, of the balano-posthitis non-ulcerated; who have made use of them as a weapon against my doctrines; who wish to prove by them that the chancre alone does not furnish inoculable pus, and that the blennorrhagia which is inoculated could not well be ulcerous—these critics can no longer make use of this argument without the new verification which its author believes indispensable. These false pustules are but little developed; most commonly they are only simple bullous elevations, beneath which, we find a superficial vesication of the skin. Here, there is not that boring of the skin, as if done by a punch, like what is observed in true inoculation. In some very rare cases, deeper inflammation might appear and produce something analogous to the furuncle. But even in these cases, the progress is always very rapid, the duration slight, from three to five or six days at most, and the healing follows also very quickly without the intervention of any treatment.

However it may be, I have said, and I persist in saying, that when the inoculation has succeeded, it is always by a pustule that the chancre commences; this is what is incontestable, and which can be re-produced at will and with certainty.

However, the writers upon syphilis, who have ranged among the primitive accidents so many phenomena which ought not to hold place among them, might have well placed here this ecthyma developed under the conditions that I have already marked out to you.

It is true that our learned colleague M. Cazenave says that the ec-

thyma may be sometimes primitive. He even cites in his treatise upon syphilitic eruptions, a very beautiful example of primitive ecthyma of the lip, the direct and immediate consequence of contagion. But what M. Cazenave says of this case, for me so frequent and common, proves to me exactly that neither Biett nor himself have known either the true nature or the essence of this accident. Read again that passage of M. Cazenave, and you will be convinced that he does not consider, in this particular case, the ecthyma as being only a period of the chancre. For him, the ecthyma which he calls *primitive*, is always a syphilitic eruption; that is to say, the product of a general infection, constitutional—in a word, what I call *secondary symptoms*. But in order to establish that the ecthyma is always the result of a previous general infection, although this might be the only isolated accident by which the syphilis commences; in order to confound the chancre with ecthymatous commencement, the true primitive ecthyma, *contagious, inoculable*, with the constitutional secondary ecthyma; M. Cazenave, after having so well said that this accident could be the first and only result of the contagion which, “apart from the influence of the virus, has need, in order to be developed, of finding particular conditions,” conditions which finally are those *which necessitate the inoculation of the primitive accident*; M. Cazenave, I say, wishing, against his own principles, to place ecthyma among syphilitic eruptions, gives as examples of primary pustular eruptions, two observations where this accident was perfectly secondary, and regularly preceded by a primary accident upon the fingers.

This error is very common among those individuals who do not know all the varieties of the chancre. Did not this happen to one of our unhappy colleagues to whom M. Cazenave makes allusion? Has he not been considered as having undergone a constitutional infection, *d'emblée*, and as having offered an example of primary pustular eruptions? And yet this unfortunate colleague had had a chancre upon one of the fingers of his right hand, a chancre followed later by an enlargement of the glands about the elbow, in the desired and regular order of secondary accidents. All this I have myself established, and also my learned friend M. Nélaton. It is true that a person who has not a very great experience with venereal maladies, although he had written much upon the subject, and who knew of the ulcerations upon the finger, has pretended that there was nothing there but an *anatomical* tubercle, which had given passage to the virus without becoming inoculated. I much fear that the brain of this person has given passage to this fine story without becoming inoculated in passing, with a little probability and good sense.

I have not yet finished with the primary ecthyma. You who read all—sometimes from duty, often from taste, and always with profit to those who read you in their turn—you ought to be surprised to see in a *manual* upon syphilitic maladies, that the learned author, whom we both hold in high esteem, admitted the possibility of the production of a pustule by artificial inoculation, but not otherwise. In effect, M. Gilbert denies absolutely that the non-inoculated chancre can commence by a pustule; he assures us that it is through an error of diagnosis

that this period of chancre has been admitted. I believe that you already see upon what side the error ought to be. If you admit, I say to M. Gibert, that a pustule can be produced by the point of a lancet; agree that it does not require a great effort of imagination to find in the processes of ordinary contagion something which may act in the same manner, such as a nail, a hair, &c., without making account of other circumstances, of which you in your quality of physician, ought to receive the lowd and shameful confessions.

You see how the most distinguished observers are nevertheless subject to error. Assuredly M. Cazenave and M. Gibert know as well as I what an ethyma is, and yet how is it that they insist in referring it always to a general state, and that they deny the existence of it as a product of chancre? Why? because that theory throws too often a deceitful gauze between the observer and the matter of observation; because that it does not suffice, as another observer has just told us, to pass ten years in a venereal hospital in order to see well all that takes place there; because, alas! there are eyes which always look and which never see.

I ask pardon, my friend, for having so long occupied myself with the particular form of the chancre. Since I have done so, it is in my opinion time, at last, to come out from this *parrot's talk*, which always gives, without variation, the same characters to the primitive accident, as if it was immutable and eternal in its form. Nothing is more false and more contrary, to the observation of every day than this doctrine. The primary accident, on the contrary, presents numerous varieties either at its commencement, during its course, or later. Permit me to recall here what observation and experience have taught me.

In the most common cases, chancre commences by a superficial or deep ulceration *d'emblée*. The primary ulcer does not always destroy all the thickness of a mucous membrane or of the skin. Thus upon the semi-mucous membrane of the gland and of the prepuce, the ulceration may be sufficiently superficial to lead to the belief of an ulcerated balano-posthitis, and to justify certain successes in inoculation.

The ulcer *d'emblée* is produced, then, if the virulent pus has been placed upon a surface recently denuded, upon a bleeding wound, or, what is more difficult and consequently more rare, upon a wound in supuration. Again, we see sometimes, and this has been disputed by those who are in the habit of disputing everything, the chancre commence under the form of an abscess. Thus, the bites of leeches which become inoculated, it is true, often offer an ethymatous form; and it also happens that the virulent pus inoculates also the bottom of the bite without inoculating the borders of it; these could then become united, and enclose, so to speak, the virus which has inoculated the bottom, and this bottom then gives rise to a little virulent abscess of the sub-cutaneous cellular tissue, which when it opens, or when it is opened, presents a chancrous foyer. The fistulous tracks of the virulent pus in the sub-cutaneous or sub-mucous cellular tissue give rise to the same phenomenon.

All this is the true result of common practice and observation in my

wards of the Venereal Hospital. I well know that in this theory so simple respecting abscess—as a form and as a primary period of chancre, an argument has been sought for, in favor of the existence of the bubo d'emblée, an existence which I do not admit, and which appears a contradiction in my doctrine. But I shall return, by-and-by, to these buboes d'emblée, and in such a way as, I hope, to satisfy my opponents.

However it may be as respects these different varieties in the commencement of chancre, they have no influence upon the ulterior form which these ulcerations will take.

This point has its importance; it becomes connected with the unity or the plurality of the syphilitic virus, a question yet sufficiently obscure, or rather obscured by the vagueness and the want of precision in facts. Here is what I can say as regards myself:—

When the inoculation is made upon the patient himself, the commencement of the chancre being always similar, the ulceration which follows the inoculation takes finally the form, and offers the same varieties, as the first accident which furnished the inoculable pus. Thus, if it is from a phagedenic chancre that the pus has been taken, the ulceration will take on the phagedenic character; if from an indurated chancre, the ulceration will become indurated, &c. Here is what my own experience has shown me. But in the inoculations which have been made from infected individuals to healthy ones, have things always passed thus? We know nothing, for in the inoculations which have thus been practised by other experimenters, they have taken note neither of the form of the accident from which the pus was taken, nor of the form of the accident which has been produced; they have been contented with saying, chancre on one side, chancre on the other, without any detailed description; so that definitely these inoculations could not be of any great assistance in the elucidation of the question.

In common observation we find that one form in an individual can produce a different form in another. But as we are never strictly sure of the source from which the infection has been taken, we can dispute the results; we can suppose that the individual who has a different form, could have taken it from another source, than that which he accuses. The results of the last inoculations which have just been made from individuals infected to those who are healthy, counterbalance and cannot serve either for or against. In the observation of M. de Welz, the pus was furnished by a non-indurated chancre, and his chancres were not indurated, which circumstance in his case might depend upon a want of aptitude. In the case of the inoculation upon the interne of the Hospital du Midi, the chancre became indurated, and yet the pus with which he was inoculated ought to come from a primary non-indurated ulcer, considering the conditions of the anterior constitutional syphilis under the influence of which the patient labored.

You see, my friend, that this question of the plurality of the virus, so clearly drawn by certain English physicians, is far from being resolved. Until now, we have always the right of believing in the existence of only one virus; it appears always rational to admit that the chancre, under given circumstances, and which can be determined in advance,

commencing always in the same manner, depends upon an identical cause, the ulterior effects of which are determined by conditions in which the individual is found, upon whom they are developed. In effect, the great varieties which the primary ulcer presents at the period of progress, which are formed more or less quick, and which can be thus summed up—simple chancres; inflammatory chancres with decided gangrenous tendency; phagedenic chancres; indurated chancres—appear to find the reasons of their existence in secondary causes beyond the specific cause. I do not here give a lecture; I do not write a book upon special pathology; consequently I cannot enter into too long details. But, in order to justify my propositions, let me recall some of the assisting causes which give to the chancre such or such a physiognomy, such or such a turn or course.

For example, observation shows what the abuse of alcoholic drinks produces, particularly in hot weather. The most simple chancres under their influence become rapidly inflammatory, and the inflammation in certain regions, as about the genital organs, in a cellular tissue which becomes cedematous so easily, arrives very quickly at gangrene. The action of alcohol in these cases, of which the English have given us such fine examples, is so pronounced that we could call these ulcers "*ano-phagedeniques*."

As to the other varieties of phagedenic chancres—pultaceous, diphtheritic, serpigenous, &c.—we often find the cause of them in certain hygienic conditions, unhealthy habitations, bad nourishment, want of cleanliness; in the unseasonable employment and abuse of rancid mercurial ointment in the dressings; in certain diathetic conditions, tubercles, scrofula, herpetic condition, scurvy, and frequently in the different conditions which favor the production of hospital gangrene. Let us add to this, as we shall see later, the influence of a former syphilitic diathesis.

However, the conditions most interesting to understand, those which constitute almost in themselves the verole, are those which preside over the *induration of the chancre*.

But the *indurated chancre* being one of the important points of the doctrine which I maintain, and which these letters are intended to defend, you will permit me to make it the subject of my next letter.

Yours, &c. RICORD.

DR JOHN C. COLBY, OF FRANCONIA, N. H.

[Extract from an Address delivered before the White Mountain Medical Society, at Littleton, Jan. 26, 1853, and communicated for the Boston Medical and Surgical Journal.]

BY ADAMS MOORE, M.D.

SINCE the last semi-annual meeting of our Society, some of us have been called to perform the solemn duty of making a post-mortem examination of the body of one of our members, John Calvin Colby, M.D. He was not one of our oldest members; he had hardly completed the term of middle age; he had just entered upon his fiftieth year of life,

and the twenty-fifth year of his professional duties, when, without notice, as it were, he was removed from this to a higher state of existence.

Three days before his death he was active in the duties of his profession. He was always active, always unwearied in his attendance upon his patients, and quick to mark the changing features of their diseases. He shared largely in the confidence of his acquaintance; and no community could feel more bereaved, by the death of one man, than the town of Franconia felt when bereft of Dr. Colby.

In all outward appearance, no man of his years had a better prospect of reaching old age. The sudden termination of that prospect produced a shock that was overwhelming to his family, and spread a sadness and gloom over the region of his professional labors.

The manner of his death was unusual. I am not aware that any death of the kind has ever occurred within the observation of any of us. It was from active hæmorrhage from the stomach. The blood issued from an artery opened by a small ulcer.

Andral, the French pathologist, said that "such cases are extremely rare, and not more than five or six well-authenticated cases are to be found on record."

Dr. Geo. Burrows, the writer of the various articles on hæmorrhage from the different organs, in the "Library of Practical Medicine," quotes a few cases from English physicians. One of them is from Dr. Carswell's work on the "Elementary forms of disease." It was a case of fatal bleeding from the stomach, on account of an opening of the coronary artery by an ulcer. The scars of several others, that had healed, were found on the coat of the stomach. No suggestion was made as to the cause of the disease. It shows one important fact, which is, the occasional healing of ulcers of the stomach.

Another of these was noticed by Dr. Latham, in a patient who came under his care, while connected with one of the English Hospitals. The subject of it was a middle-aged man, who admitted that he was in the habitual use of alcoholic drinks. For the last two years, he had felt frequent pains across the lower part of his chest; often vomited his food; had palpitations of the heart, constipation of the bowels, and a pale and dusky countenance. Two days before Dr. Latham saw him, he was seized with giddiness and faintness, and vomited two quarts of blood. He lived three days longer, and each day had more or less vomiting and purging of blood. Upon the post-mortem examination, there was found a small excavated ulcer, with hardened edges, at the lesser arch of the stomach. In the base of this ulcer were seen the orifices of two or three branches of the coronary artery laid open by ulceration.

One other case, in the same hospital, was recorded, where the ulcer extended through the coats of the stomach, into the pancreas, and opened a branch of the splenic artery, from which fatal hæmorrhage ensued.

Dr. Colby had often spoken of being conscious of an adhesion of his lungs, which so appeared on the examination of his chest. The right lobe of the lungs adhered to the diaphragm and pleura of the ribs to a very considerable extent, and the left lobe was slightly adherent to the

diaphragm. These adhesions were the result of a pleurisy more than twenty-five years ago.

The stomach contained a coagulum of blood, completely moulded to its form, of the bulk of three pints or more, with a small quantity of dark fluid, commingled with the natural secretions of the organ. The coats of the stomach were pale, and its vessels bloodless. On the posterior portion of the inner coat of the stomach, two and one half inches from its upper orifice, was an excavated ulcer of the size of an English shilling, with its edge on one side considerably elevated, and indurated, and in its base two openings quite through its coats. One of these was of the size of a buck-shot, where the open point of an artery was visible. The other perforation was of half that size. The diseased portion of the stomach adhered to the diaphragm, so as to prevent any escape of its fluid contents.

In stature, Dr. Colby was full six feet. When young, he was slim. At the time of his death his weight was over two hundred pounds. His temperament was strongly marked as sanguine. He rigidly abstained from alcoholic beverages. He always had a good appetite for food. His digestive powers were strong, and he ate freely of all the luxuries of the table, including raw apples, and drank freely of strong coffee. Formerly he chewed and smoked tobacco, which he ceased to use about ten years ago, since which time he grew more and more corpulent every year.

For the last year or more he had been subject to occasional paroxysms of pain at the lower part of the chest, more frequently after eating, and in the recumbent position.

On Friday, Dec. 3d, 1852, he travelled in his carriage about fifteen miles. On that day he felt some fullness about the stomach, and passed some blood from his bowels. He had the same symptoms the next day, and did business only at or near his office. Early on Sunday morning, the 5th, he was seized with vomiting, and ejected a large quantity of blood, both coagulated and fluid, followed by faintness if he attempted to rise. Pulse in the forenoon about 100; in the afternoon less frequent, and the patient felt much more comfortable. On Monday morning he arose, felt a good appetite for food, and partook of some chicken. At 5 o'clock in the afternoon, he vomited largely of blood, and again at 11 o'clock, the same evening. He was restless and faint until 1 o'clock next morning, when he slept, and remained quiet until about 5 o'clock in the afternoon, when he again vomited largely as before. He did not vomit again, but gradually sank, and died at half past 5 o'clock next morning.

Of the professional standing of Dr. Colby, in the estimation of the community where he was known, I cannot speak in too high terms of praise. Great confidence was placed in him. His people have no hope of finding a physician to fill his place. If he had foibles or faults (and where shall we find the physician who has none), they were few and small in the eyes of his friends, compared with the excellent traits of his character as a man, a christian and a physician.

By his professional brethren he could not be regarded as a man of

great science, or a great master of medical books, but all accorded to him the reputation of a great practitioner of medicine. He studied his cases well, so far as could be done by his own powers of observation and reflection. I often met him in the sick-room, many times in cases of difficulty and doubt, and always found he had a large fund of experience to draw upon. His powers of perception, of arrangement, and of communication, were remarkably good.

PERMANENT CURE OF REDUCIBLE HERNIA.

[Continued from page 83.]

ACUPUNCTURE has been tried to a very considerable extent both in Europe and this country. Two or three rows of punctures were made through the integuments and the neck of the sac, just below the external ring, with a common needle of the ordinary size, or an acupuncture needle prepared for the purpose. There is no reason to believe that any permanent good effect has been produced in this way, and it is not probable that any one tries this method at the present time with the expectation of producing by it a radical cure of hernia.

The same may no doubt be said of the *scarification of the inguinal canal*, as practised by M. Velpeau a few times, and the *sub-cutaneous scarification of the neck of the sac*, as performed by M. Guerin. Besides the utter inefficiency of these operations, there is, especially in the former, some danger of wounding the epigastric artery.

The operation by *injection* has been done in two ways. In one, the neck of the hernial sac is previously laid open, and the fluid then thrown in; and, in the other, it is introduced by the sub-cutaneous method. The first is the operation as performed by M. Velpeau, and the other that of Dr. Pancoast, of Philadelphia.

M. Velpeau was evidently dissatisfied with all the operations that had been performed for the radical cure of reducible inguinal hernia; but he was unwilling to believe that no remedy could be found for it. The success which so often followed the operation for hydrocele by injection, led him to think that a similar course might produce the same results in the treatment of reducible hernia.

He accordingly performed the operation on the first favorable case that presented. An incision of an inch in length was made just below the external ring down to the neck of the sac; this was opened with a bistoury, and a mixture of six drachms of tincture of iodine in three ounces of water was thrown in. An assistant compressed the inguinal canal, so as to prevent the fluid from coming in contact with the peritoneum above the ring. After the injection had been pushed around the various parts of the sac, it was allowed to escape through the canula. No unpleasant symptoms followed; but the final result of the experiment has not, as far as we know, been made public.

M. Velpeau does not seem to have much confidence in the operation, and it is understood that he does not continue to perform it at the present day. He has probably learned that something more than the mere

closure by the process of adhesion of the neck of the sac is necessary for the radical cure of hernia. The fibrin that is effused will in most cases be soon absorbed, so that the barrier which had been relied on to prevent the descent of the hernia will be entirely removed.

About the same time, Dr. Pancoast performed the operation which is described in his work on "Operative Surgery." The hernial sac, its contents having been previously returned, was punctured with a small trocar passed through a canula. Having ascertained that the instrument was fairly in the sac, by the freedom with which it could be moved about, the point of it was then directed upwards so as to scarify the internal surface of the upper part of the sac. The trocar was then withdrawn, and half a drachm of the tincture of iodine, or an equal quantity of the tincture of cantharides, was thrown in slowly by means of a small syringe fitted to the canula. The canula was then withdrawn, and a compress was applied just above the external ring, and the pad of the truss, which had been on before the operation, was brought down over the compress.

This operation was performed in thirteen cases, in one of which only were there any symptoms of serious inflammation, and these readily yielded to leeches and fomentations. On some of these patients a single operation was performed, and on others, in whom the sac was large, several were required. All of them were evidently benefited at the time, but whether a radical cure was effected in any instance could not be ascertained, as nothing was known of the patients after a few months from the time of the operation. Whether Dr. Pancoast continues to practise it, we are unable to say.

This method has, in the opinion of the Committee, all the advantages of that of M. Velpeau, while it avoids in a great degree the danger of peritoneal inflammation, to which patients are exposed by his mode. When the hernial sac is laid open, there is, of course, a direct communication between the abdominal cavity and the external wound. This alone would be likely to excite inflammation, and if, in addition, a part of the peritoneum is subjected to the action of an irritating fluid, there is reason to fear that the inflammatory process would not be limited to the sac, but that fatal peritonitis would be the consequence.

Admitting that these operations accomplished all that they were designed to do, it does not follow, by any means, that they would in every instance produce a radical cure. All that they could effect, if successful, would be to close the neck of the sac, without contracting the tendinous opening or ring. Sir Astley Cooper very truly says—"that, although the original sac may be completely shut at its mouth by adhesion or perfect contraction, it is possible that another sac may be formed contiguous to the first." It fact, it is well known that sometimes the hernia has recurred, after the whole of the original sac has been removed. Contracting or even closing the neck of the sac is evidently then not enough; "something more," says Mr. Lawrence, "is required; we want a remedy that should contract the tendinous opening; for while that remains preternaturally large, a new protrusion is a highly probable occurrence."

This has been attempted in two ways. The first is by scarification of the external ring in inguinal hernia, and the other is by means of sutures. The first of these is quite an old operation. Heister says that —“Some surgeons scarify the ring of the abdomen, or aperture through which the intestine prolapsed, together with the skin, in order to render the cicatrix more firm; by which means many have been cured of these ruptures, especially if they continue to wear a proper bandage for a considerable time afterwards. But I think that the operation may succeed better in infants than in adults.”

It is perhaps enough to say, with regard to this method, that it has been occasionally tried from time to time, for more than a hundred years, without a sufficient degree of success to gain the confidence of surgeons; and it is not to be overlooked that the danger of wounding the epigastric artery is no inconsiderable one; enough at any rate to deter all but the most expert from attempting to perform it.

[The length of the articles in the preceding pages prevents the insertion, this week, of the concluding part of the report on hernia.]

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON, MARCH 2, 1853.

Beauties and Deformities of Tobacco-Using. — A formidable array of medical as well as clerical talent has been gradually collecting in New England, within the last few years, against the use of tobacco. While one party sets forth its demoralizing tendencies, the other pours in broadsides of double-shotted arguments to prove its destructive effects upon health. Whether the consumer smokes, chews or snuffs, it is all the same to the great school of reformers, for they give no quarter to a man who in their opinion has no mercy on himself. Dr. L. B. Coles, a fellow of the Mass. Med. Society and of the Boston Medical Association, has been laboring indefatigably, for some years, to revolutionize the public sentiment in respect to the habitual use of tobacco. Before us is a revised edition of a book from his pen, bearing the title of *Beauties and Deformities of Tobacco-Using*—in which the author pleads, with undiminished ardor, against the use of the vile weed. Dr. Coles has exhausted all the usual arguments in sustaining his positions, so that were we ever so much inclined to give him assistance, there is nothing left for us to say, without resorting to his own tropes and striking figures. We may say, however, that we hope the heaven which he has sown is operating favorably, and that the next generation will grow up without the odor of tobacco in their garments. There is no prospect of the present hardened race, if the actual consumption of Havana cigars and the best cavendish is any criterion of the inveteracy of the habit. We are certainly a smoking, chewing people. Our intense nervous activity finds some imagined relief in this exercise of the jaws; and as for health, it is not of the least consequence, the country being full of patent remedies for all kinds of diseases. There was a period in the history of Massachusetts, a State that has always been celebrated for legislating upon every thing, when no person could chew tobacco without a license from

one of the Judges of a court—and his honor was restricted from granting it, without the certificate of a physician that it was necessary to the health of the applicant. Moral suasion is now the power that is brought into requisition, but it acts too tardily and too feebly to correct the evil. The age of bronze, of iron and of gold, may be considered to have passed by, and we are now living, as it were, in the age of tobacco. What an epoch to reckon from! Smoke and fume seem to gather round our heads as we write. Imagination, like the Witch of Endor, calls up the ghastly, saffron-colored wretches who have died by inches, holding on to the pipe-stem. And yet tobacco smokers are the terror of railroad corporations and public-house keepers all over the country. "*No smoking here*," is universally posted on the walls of depots, and incorporated into all travelling regulations. The tobacco man is haunted at every nook and corner of society; but he remains obdurate, still smoking and chewing, and we fear he will continue in his evil ways till he reads Dr. Cole's expose of his last earthly condition! It is a sad picture, but a true one. We believe, with Dr. Coles, that the less any one has to do with tobacco, the better he will find himself in the end. With these considerations, we recommend his "*Beauties and Deformities of Tobacco-Using*," to the special cognizance of those unfortunates for whom it was designed.

Scalds.—The popular belief that cold flour is a good remedy for scalds, has been often verified of late. The relief is almost instantaneous. Plunge the limb, if scalded, into a pan of flour. Simply dredging the burned surface, answers the same purpose; and it is not improbable that a burn would be benefited by the same treatment. One of the latest testimonials in favor of the flour, is the following, which was taken from a respectable exchange paper.

"While at the supper table, a child, which was seated in its mother's lap, suddenly grasped hold of a cupful of hot tea, severely scalding its left hand and arm. I immediately brought a pan of flour and plunged the arm in it, covering entirely the parts scalded with the flour. The effect was truly remarkable; the pain was gone instantly. I then bandaged the arm loosely, applying plenty of flour next to the skin, and on the following morning there was not the least sign that the arm had been scalded, neither did the child suffer the least pain after the application of the flour."

Prevalence of Smallpox.—With a positive preventive, the world has seldom been more severely afflicted with that most loathsome of all diseases, smallpox, than at the present moment. It is customary in almost every place to defer vaccination till smallpox is at the door. On the four great continents, it is now scourging the inhabitants in its most terrific forms; but the people will not generally avail themselves of the sure preventive. At Zanzibar and the regions about, at the last advices, the disease was mowing down every age and sex. "The entire population, with the exception of the Europeans, paid their tribute to the terrible scourge, the germ of which was brought from Muscat by a vessel of the Imaum. In the Persian Gulf and in that of Oman, where cholera had already raged, thousands of victims have succumbed. The disease has spread with frightful rapidity, not only along the eastern coast of Africa, but also in the interior of that continent."

Provisional Callus in Fractured Bones.—Dr. F. H. Hamilton's new views on this subject, in a pamphlet form, from the pages of the Buffalo Medical Journal for February, have been received. Dr. H. is an instructive writer on surgery, and is equally distinguished for a tact in teaching that branch of the profession. Although we have no space to copy any part of this essay, we cannot refrain from recommending the study of our friend's able performance to surgical operators. The opinions of a skillful, close observer, on a subject so important as the fracture and re-union of bones, cannot be otherwise than profitable to all young beginners; and in this instance, the old will find themselves profited.

Reports of Births, Marriages and Deaths in Boston, for 1852.—This document, by the City Registrar of Boston, Artemas Simonds, Esq., contains the requisite tabular statements concerning the vital statistics of our city during another year. It is prepared with the faithfulness characteristic of the registry department under its present management. We have space, this week, for only a few of the sums total of the different abstracts. There were registered, during the year, 5,308 births—viz., 2,651 males, and 2,657 females. These were children of 1,681 American fathers, and 1,733 American mothers; and of 3,479 fathers and 3,451 mothers of foreign nativity—the birth places of 96 fathers and 72 mothers being unknown. Certificates of marriage were issued to 2,877 couples, and 2,686 marriages were recorded. The deaths registered were 3,736; 1,902 males and 1,834 females—1,568 being of American and 2,168 of foreign origin. Of the interments during the past year, it is gratifying to find that 2,177 of the 3,736 who died in Boston, were buried out of the city.

Motorpathy.—A new book, heralding a new theory, has originated at Rochester, N. Y., having the following title—“*Exposition of Motorpathy: a new system of curing disease, by statuminating, vitalizing motion.*” By H. Halsted, M.D. These are hard terms for a new disciple to articulate. Eleven chapters of the above-named work are devoted to the consideration of uterine diseases, all of which appear to yield readily to the statuminating process. An irresistible mass of evidence is introduced in illustration of the value of motorpathy in the treatment of females who have been received at the discoverer's institution, known as Halsted Hall. The kind of medication in practice there, as nearly as can be gathered from the volume under consideration, does not differ essentially from methods perfectly understood throughout Christendom. “Among these therapeutic agents, that part of motorpathic treatment given personally, which is a process of statuminating vitalization, stands at the head. Diet, the use of water, magnetism, dry-cupping and various modes of exercise, and medication by external application, are resorted to, when the occasion demands, as aids to a more speedy realization of the objects proposed.” Although the work is well written, and the author, we should think, might safely be called a literary man, it is difficult for the reader to divest himself of the suspicion that the plan contemplated by its publication is simply the circulation of an advertisement, to drum up customers for Halsted Hall.

Veratrum Viride—Typhoid Fever of the South. MR. EDITOR.—We do not like to be a fault-finder, nor do we repudiate any thing without a fair

trial, but we think writers in promulgating the virtues of any new agent, should keep upon the line of medical knowledge, and not wander into the mists of untenable sophisms and vague speculations. These remarks are elicited by reading an article in your issue of Feb. 9th, extracted from the South. Med. and Surg. Journal, by Dr. W. C. Norwood, of South Carolina, upon the virtues of Veratrum. Dr. Norwood is a clever man; we appreciate his zeal, sympathize in his misfortunes, and admire his energy; but we beg leave most respectfully to dissent from his opinion with regard to veratrum in Typhoid Fever; and we believe that in this position, we are sustained by at least three fourths of southern practitioners, who are men of practical experience and extensive observation. That veratrum will control arterial excitement, we believe nobody south denies; but that it is applicable to uncomplicated Typhoid Fever, as a general remedy, or that it will cure it, very few think. The nature and character of Typhoid Fever, it appears to us, must ever preclude the use of veratrum as a general agent, in its simple forms. If Typhoid Fever was only an excited state of the heart and arteries, then, we readily grant, veratrum would cure it; but as long as pathology teaches us different lessons, we must search for another remedy. We do not by any means conceive, that veratrum will cure Typhoid Fever, Pneumonia, or other febrile affections, merely because it will curb the velocity of the circulatory apparatus. Digitalis is an arterial sedative, but nobody pretends to claim for it curative powers in Typhoid Fever, Pneumonia, or fevers generally. While we admit veratrum is an arterial sedative, we do not consider it a curative agent in southern Typhoid Fever, but only an adjuvant of doubtful and hazardous powers, in its influence and effect. There is a form of cardiac disease, following Typhoid Fever in its convalescence, in which we have prescribed veratrum with magic results. We believe it goes well in southern Pneumonia, and in cardiac affections of a sthenic character; but in simple Typhoid Fever characterized by nervous mobility, we regard it not only a questionable but a *peculiarly dangerous agent*.

Should these remarks reach the eye of Dr. Norwood, we beg him to believe us a friend; we admire his medicine for its real virtues, but fear the misguided opinions of others, who are as honest as ourselves, but not tempered in "*veratric*" zeal, will cause much odium to be cast upon the preparation by attributing to it panaceal powers which can never be realized. In conclusion, we reiterate, if veratrum will cure southern Typhoid Fever, the physicians of the South have mistaken its pathological character; and autopsal results, as well as perspective causes, are dead letters in science.

"SOUTHERNER."

P. S. We refer the reader to Dr. Pendleton's cases, Dr. Wiekess' notes, and others, in the Charleston Review and Nelson's Lancet. These cases, to us, would satisfy any candid man.

MARRIED.—In Charlestown, Feb. 17th, Dr. J. C. Dorr, of Stoneham, to Miss A. Malvina Flint, of Charlestown.

Deaths in Boston—for the week ending Saturday noon, Feb. 26th, 78.—Males, 37—females, 41. Disease of the brain, 2—congestion of the brain, 2—consumption, 17—convulsions, 2—croup, 4—cyanosis, 1—dysentery, 1—dropsy, 4—drowned, 1—infantile diseases, 3—puerperal, 2—erysipelas, 1—typhoid fever, 1—scarlet do., 7—fracture, 1—hooping cough, 2—disease of the heart, 6—disease of the kidney, 3—inflammation of the lungs, 9—disease of liver, 1—marasmus, 1—old age, 2—peritonitis, 1—palsy, 1—pleurisy, 1—rheumatism, 1—teething, 1.

Under 5 years, 27—between 5 and 20 years, 11—between 20 and 40 years, 21—between 40 and 60 years, 11—over 60 years, 8. Born in the United States, 54—Ireland, 19—England, 2—Scotland, 1—Br. Provinces, 1—Denmark, 1.

Prizes Awarded by the Academy of Sciences of Paris.—These very important encouragements to the laborers in the field of medical science, were awarded at the meeting of the 20th of December, 1852. Among the authors who were rewarded, we notice—Dr. Budge (an English physician) and Dr. Waller, of Bonn, for physiological researches; M. Lebert, for his work on cancer, and the curable affections confounded with cancer; M. Davaine, for his memoir on the paralysis of the seventh pair of nerves on both sides; and on the influence of the facial nerves upon the movements of the soft palate, the pharynx, and the tongue; M. Bretonneau, for having introduced the operation of tracheotomy in croup; and M. Trousseau, for having improved and simplified the same operation. M. Niepce also obtained a prize for his researches on cretinism; and M. Renault, professor at the Veterinary School of Alfort, had a prize allotted to him for his investigations, both practical and experimental, on the effect produced by the ingestion of virulent matter into the digestive canal of man, or the domestic animals. M. Renault has found that the virulent fluids have no influence on the intestinal tract of domestic animals, and that their flesh does not, by such ingestion, become unwholesome.—*London Lancet.*

The Medical Profession in Paris.—The Medical Directory of Paris, published by *L'Union Médicale*, gives the following numbers as to our Parisian brethren. Doctors of medicine and of surgery, 1337; officiers de santé (an inferior grade), 179; pharmaciens, 423; midwives, 277. From the 1st of January, 1851, to 31st December, 1852, there died in Paris 39 doctors of medicine; in the two previous years 64 had died. In the year just elapsed, 88 new practitioners set up in the capital. This year's list contains 15 medical men less than the last. The Directory also gives the numbers in the districts surrounding Paris, and from these statements it would appear that there is a great disproportion between doctors and patients. There are in fact less than 500 inhabitants for one medical man; and when it is considered how many of these apply to public institutions, very little is left for individual practitioners. *L'Union Médicale* warns young men from settling in Paris, as the exuberance of professional men is enormous.—*Ibid.*

Dr. Drake's Work on the Diseases of the Interior Valley of North America.—We are happy to learn that Dr. Drake had prepared a considerable portion of the second volume of this great work, and that it is the desire and intention of his family that the portion so prepared should not be lost to the Profession. It is to be revised by a competent person, and printed as soon as possible.—*Southern Med. and Surg. Journal.*

Ligatures of Large Arteries. by Prof. Roux.—M. Roux, the Nestor of French Surgery, occasionally furnishes the statistics of his extensive experience. In a paper communicated to the Chirurgical Society we find that he has ligated the popliteal artery once, the femoral 46 times, the brachial 20 times, the carotid 6 times, the axillary 4 times, the subclavian 3 times, and the external iliac twice—making 82 operations.—*Ibid.*

The new building for the London Hospital for diseases of the chest, Liverpool street, Finsbury, is hastening to completion, and it is said will be opened in June.